

PHILOSOPHY, SCIENCE, AND PSYCHOANALYSIS

A CRITICAL MEETING



EDITED BY
SIMON BOAG, LINDA A.W. BRAKEL,
AND VESA TALVITIE

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PSYCHOANALYSIS
A Critical Meeting

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Linda A. W. Brakel, and
Vesa Talvitie

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INTRODUCTION

Vesa Talvitie, Linda A. W. Brakel, and Simon Boag

The perennial interest in psychoanalysis shows no signs of abating, and the longevity of psychoanalytic theory is seen in the varied extensions and elaborations of Freudian thinking in the fields of neuroscience and cognitive theory. The enduring interest in psychoanalysis is, in many respects, understandable: psychoanalytic theory addresses such issues as unconscious mental processes, self-deception, and wish-fulfilment, and makes bold claims in terms of using these concepts to explain both everyday behaviour and clinical phenomena. Nevertheless, while recent developments in mainstream psychology have repackaged many of Freud's ideas (demonstrating the vitality of Freud's thinking), there remains doubt about the veracity of psychoanalytic claims, and questions concerning the place of psychoanalysis vis-à-vis science. Furthermore, developments in the fields of philosophy, psychology, and the cognitive—and neurosciences—since Freud's time also require consideration with respect to appreciating their implications for contemporary psychoanalysis. Such considerations may have important practical implications, since psychoanalysis is not simply concerned with theory for theory's sake and instead considers the implications of theory for therapeutic practice. However, assessing psychoanalysis in light of modern-day research is not an easy project, not least because the complexity of psychoanalytic theory raises complex scientific and philosophical questions concerning the nature of mind and the nature of the scientific enterprise itself. Accordingly, a fresh evaluation of psychoanalysis in the new millennium entails a perspective that is at once scientific and philosophical and represents the junction where philosophy, science and psychoanalysis meet.

This book aims to provide a forum within which discussions of psychoanalysis and psychoanalytic research go beyond partitioning philosophy and science, and sees, instead, a rigorous science as being inherently philosophical in nature. But the first obstacle in any discussion here is that the topic of philosophy, science and psychoanalysis branches in many directions. It would be desirable that a book on this topic interests both psychoanalytic audiences and “outsiders” to the field. This is, however, a rather difficult criterion to meet for many reasons. The title *Philosophy, Science, and Psychoanalysis* covers a wide range of topics, and no one person will be fully acquainted with or interested in them all. Some topics are, for want of a better term, “solipsistic”—of primary interest only to a psychoanalytic audience. Counter to that, other topics deal with nuances of philosophical issues, and a clinically oriented reader, for instance, may find both following these issues and appreciating their significance difficult. Additionally, readers will all arrive at the material presented in this volume from their own explicit or implicit philosophies, the assumptions of which may hinder communication.

and appreciation for the positions presented here.

Nevertheless, it is important not to avoid this kind of challenge. Due to the nature of Sigmund Freud's work, a wide range of topics including both abstract scientific and philosophical issues are embedded within psychoanalytic thinking. Thus, as long as psychoanalysis contains relevance for people, we must keep on diving into those deep waters of science and philosophy. The situation is somewhat circular: if psychoanalysis withdraws from studying topics such as those treated in this volume, it loses its relevance to the academic community.

The importance of the history of psychoanalysis

Due to the enduring respect for the founder's writings, one cannot make sense of psychoanalysis' (prevailing) relations to science and philosophy without knowing the zeitgeist of the late nineteenth-century middle Europe where German romanticism still had a notable impact on people's thinking. Considering the topic of philosophy, Freud, as a young man, and his contemporaries read such notable figures as Kant and Hegel. Despite appearances to the contrary in his later life, Freud enjoyed reading philosophical writings in his youth, and only later developed a dismissive stance toward the discipline. In fact, Alfred Tauber (2011) describes Freud as a "reluctant philosopher" (see however, Boag, 2011a). When analytic philosophy emerged, Freud was in his fifties and subsequently some would say that Freud and his contemporaries were not terribly interested in such questions as "how should the term 'mental' be defined?"—even if he did offer a subtle and effective philosophical argument against his detractors, who claimed that psychological processes and contents must be conscious, *by definition* (Freud, 1915e). It was not until more than a decade after Freud's death that philosophers began to talk about (Wittgensteinian) language games. Thus the question concerning the relation between language and the world, and more generally the entire philosophy of language, were not particularly vivid for Freud, even if he believed that language was necessary for consciousness (e.g., Freud, 1900a, 1915, 1940a[1938]). Additionally, when reading Freud and talking about his ideas we easily bypass the above matters, as we tend to downplay the significant temporal distance between us and Freud. For most readers, Kantian thinking and positivism appear as opposite and incompatible philosophical orientations. However, it seems that Freud had no difficulty in possessing sympathies toward both of them (Tauber, 2011), a trait also reflected in Freud's acceptance at various points in his writings of contradictory positions generally (for instance, see Petocz's (2006) discussion of Freud's discussion of the mind-body relation). Had Freud paid greater attention to philosophical issues then possibly there would be fewer disputes and disagreements concerning Freudian theory today.

On the other hand, with respect to scientific issues, the temporal distance is also salient in the case of Freud's appreciation of evolutionary theory. When Darwin's *Origin of Species* was published in 1859, Freud was three-years old. It is easy in retrospect to attribute a contemporary (Darwinian) understanding of evolution to Freud's thinking, but, as generally known, Freud also retained an adherence to Lamarckian lines of thought until the end of his life (see Jones, 1957, pp. 310–311; let us mention that recently certain Lamarckian-like ideas have been revived in the domain of epigenetics). Moreover, the development of psychoanalysis occurred prior to the introduction of psychopharmaceuticals. For example, in the domain of psychiatry, first generation antipsychotic drugs were developed in the 1950s and evidence-based medicine emerged at the end of the twentieth century—about half a century after Freud's death. Such factors provide some context of the world in which psychoanalysis began. On top of this, our notions of philosophy and science, and the ideals concerning them, are developing and changing all of the time, as do conceptions of psychoanalysis. For instance, in France an idiosyncratic mixture of continental philosophy and psychoanalytic ideas emerged, while in Britain, Wilfred Bion created an original conception of psychoanalysis, and in the United States

psychoanalysis took place in the context of empirical psychology and was influenced by the idea of information-processing. Psychoanalytic clinical theory and its method of cure have also experienced a number of branchings. Currently there are numerous psychoanalytical schools, whose interrelations are not always especially warm and accepting of one another.

When looking at the (big) picture painted above, it is clear that one cannot compose a definitive handbook on the relations between psychoanalytic, scientific and philosophical ideas. Nonetheless there have been some notable attempts beginning at least from the year 1959, when *Psychoanalysis: Scientific Method, and Philosophy*, edited by Sidney Hook, was published. The book is based on the presentations held in the Second Annual Meeting of the New York University Institute of Philosophy. In its preface the editor states that the seminar was probably the first in the United States where a distinguished group of psychoanalysts has met with a distinguished group of philosophers of science in a free, critical interchange of views on the scientific status of psychoanalysis" (Hook, 1959, p. xii). Following that, several edited books have been published, including *Philosophical Essays on Freud* (Wollheim & Hopkins, 1982), *The Cambridge Companion to Freud* (Neu, 1991), *Mind and Psychoanalysis and Science* (Clark & Wright, 1998), *Freud 2000* (Elliot, 1998), *The Analytic Freud*, *Philosophy and Psychoanalysis* (Levine, 2000), *The Freud Encyclopedia: Theory, Therapy, and Culture* (Erwin, 2002) and *Psychoanalysis at the Limit: Epistemology, Mind and the Question of Science* (Mills, 2004). Thus there is a relatively long-standing recognition of the importance of both philosophical and scientific issues within psychoanalysis.

The topics covered within these volumes may be classified in many ways, the simplest being according to the issues included. Also, alongside of each specific topic, the basic approach of an article may be categorised as either *intrapsychoanalytic* or *interdisciplinary*. With the former approach, psychoanalysis is treated as a more or less independent discipline possessing its own methodology and object of study. A writer aims to reveal something about a phenomenon by applying psychoanalytic theories or viewpoint. On that basis, a writer may create contributions of an aesthetic, historical, ethical, or educational nature, or of a cultural or philosophical nature without necessarily referring to studies put forward outside psychoanalysis. Alternatively, studies that may be called *interdisciplinary* are grounded on the assumption that psychoanalysis and certain other disciplines possess considerable shared interests and objectives. On one hand this implies that psychoanalytic viewpoints may enrich, for example, the biologists' or philosophers' thinking, and on the other that psychoanalytic hypotheses may be tested and evaluated with the help of the outcomes of (for example) neuroscience or cognitive psychology. Articles falling into this category may be critical toward psychoanalysis, or the author(s) may argue that non-psychoanalytic studies support psychoanalytic insights.

The other additional core topic of this book is Adolf Grünbaum's critique of psychoanalysis. Although far newer than conflicts over the status of unconscious mentation, Professor Grünbaum's views have already been debated for more than three decades. Perhaps no single contributor's criticism in the academic world has been as influential and long-standing as that of Grünbaum's. While many advocates of psychoanalysis are perhaps irritated about Grünbaum's persistent argumentation, psychoanalysis should be (also) honestly grateful to him: during past decades the discussions around Grünbaum's criticisms have been among the most notable link between psychoanalysis and the academic world. Thanks to Professor Grünbaum, psychoanalysis is less isolated from the academic world than it would be otherwise. In this volume we find Grünbaum's latest formulation of his critique; within the same spirit is Edward Erwin's criticism of psychoanalysis, and there are several reflections on the plausibility and significance of Grünbaum's and Erwin's arguments—particularly Linda A. W. Brakel's reply to Grünbaum and Agnes Petocz's discussion of the scientific status of psychoanalysis.

Psychoanalysis will of course face challenges from science and philosophy beyond those treated in this volume. Developments in neuroscience will continue to warrant re-examining the nature of psychoanalytic theories. Also, increasingly, the market place, insurance companies and public administration will all clamour for evidence-based evaluation of all psychological and psychiatric treatments. The talking-cure method of psychoanalysis cannot and should not bypass this issue. While we do not address these problems directly, we hope that this volume will have laid the groundwork for facing these future challenges.

CHAPTER ONE

Critique of Psychoanalysis*

Adolf Grünbaum

I. Introduction

The most basic ideas of psychoanalytic theory were initially enunciated in Josef Breuer's and Sigmund Freud's "Preliminary Communication" of 1893, which introduced their *Studies on Hysteria*. But the first published use of the word "psychoanalysis" occurred in Freud's 1896 French paper on "Heredity and the aetiology of the neuroses" (1896a, p. 151). Therein Freud designated Breuer's method of clinical investigation as "a new method of psycho-analysis." Breuer used hypnosis to revive and articulate a patient's unhappy memory of a supposedly *repressed* traumatic experience. The *repression* of that painful experience had occasioned the first appearance of a particular hysterical symptom, such as a phobic aversion to drinking water. Thus, Freud's mentor also induced the release of the suppressed emotional distress originally felt from the trauma. Thereby Breuer's method provided catharsis for the patient.

The cathartic *lifting* of the repression yielded relief from the particular hysterical symptom. Breuer and Freud believed that they could therefore hypothesise that the *repression*, coupled with affective suppression, was the crucial cause for the development of the patient's psychoneurosis (1895d, pp. 67, 29–30).

Having reasoned in this way, they concluded in Freud's words:

Thus one and the same procedure served simultaneously the purposes of [causally] investigating and of getting rid of the ailment; and this unusual conjunction was later retained in psycho-analysis. (1924f, p. 194)

In a 1924 historical retrospect (1924f, p. 194), Freud acknowledged the pioneering role of Breuer's cathartic method:

The cathartic method was the immediate precursor of psychoanalysis; and, in spite of every extension of experience and of every modification of theory, is still contained within it as its nucleus.

Yet Freud was careful to highlight the contribution he made himself after the termination of his collaboration with Breuer. Referring to himself in the third person, he tells us:

Freud devoted himself to the further perfection of the instrument left over to him by his elder collaborator. The technical novelties which he introduced and the discoveries he made changed the cathartic method into psycho-analysis. (1924f, p. 195)

These extensive elaborations have earned Freud the mantle of being the *father* of psychoanalysis.

By now, the psychoanalytic enterprise has completed its first century. Thus, the time has come to take thorough *critical* stock of its past performance qua theory of human nature and therapy, as well as to have a look at its prospects. Here I can do so only in broad strokes.

It is important to distinguish between the validity of Freud's work qua *psychoanalytic* theoretician and the merits of his earlier work, which would have done someone else proud as the achievement of a life-time. Currently, Mark Solms, working at the Unit of Neuro-surgery of the Royal London Hospital (Whitechapel) in England, is preparing a five-volume edition of *Freud's Collected Neuroscientific Writings* for publication in all of the major European languages. One focus of these writings is the neurological representation of mental functioning; another is Freud's discovery of the essential morphological and physiological unity of the nerve cell and fibre. They also contain contributions to basic neuroscience such as the histology of the nerve cell, neuronal function and neurophysiology. As a clinical neurologist, Freud wrote a major monograph on aphasia (Solms & Saling, 1990). As Solms points out in his preview *An introduction to the neuro-scientific works of Sigmund Freud* (unpublished), Freud wrote major papers on cerebral palsy that earned him the status of a world authority. More generally, he was a distinguished pediatric neurologist in the field of the movement disorders of childhood. Furthermore, Freud was one of the founders of neuro-psychopharmacology. For instance, he did scientific work on the properties of cocaine that benefited perhaps from his own use of that drug. Alas, that intake may well also account for some of the abandon featured by the more bizarre and grandiose of his psychoanalytic forays.

As Solms has remarked (private conversation), it is an irony of history, that Freud, the psychoanalyst who postulated the ubiquity of bisexuality in humans, started out by deeming himself a *failure* for having had to conclude that eels are indeed bisexual. In a quest to learn how they reproduce, one of Freud's teachers of histology and anatomy assigned him the task of finding the hitherto elusive testicles of the eel as early as 1877, when he was 21 years old. After having dissected a lobular organ in about 400 specimens in Trieste, Freud found that this organ apparently had the properties of an ovary no less than those of a testicle. Being unable to decide whether he had found the ever elusive testicles, Freud inferred that he had failed, as he reported in a rueful 1877 paper.

In 1880, he published a (free) translation of some of J. S. Mill's philosophical writings (Stephanou, 1989, pp. 85–86). Yet he was often disdainful of philosophy (Assoun, 1995), despite clearly being indebted to the Viennese philosopher Franz Brentano, from whom he had taken several courses: The marks of Brentano's (1874) quondam representationalist and intentionalist account of the mental are clearly discernible in Freud's conception of ideation. And the arguments for the existence of God championed by the quondam Roman Catholic priest Brentano further solidified the thoroughgoing atheism of Freud, the “godless Jew” (Gay, 1987, pp. 3–4).

II. History and logical relations of the “dynamic” and “cognitive” species of the unconscious

Freud was the creator of the full-blown theory of psychoanalysis, but even well-educated people often don't know that he was certainly *not at all* the first to postulate the existence of *some kinds or other unconscious mental processes*. A number of thinkers did so earlier in order to explain conscious thought and overt behaviour for which they could find no other explanation (1915e, p. 166). As we recall from Plato's dialogue *The Meno*, that philosopher was concerned to understand how an ignorant

slave boy could have arrived at geometric truths under mere questioning by an interlocutor with reference to a diagram. And Plato argued that the slave boy had not acquired such geometric knowledge during his life. Instead, he explained, the boy was tapping prenatal but *unconscious* stored knowledge, and restoring it to his conscious memory.

At the turn of the 18th century, Leibniz gave psychological arguments for the occurrence of *sub-threshold* sensory perceptions, and for the existence of unconscious mental contents or motives that manifest themselves in our behaviour (Ellenberger, 1970, p. 312). Moreover, Leibniz pointed out (1981, p. 107) that when the contents of some forgotten experiences subsequently emerge in our consciousness, we may *misidentify* them as *new* experiences, rather than recognise them as having been unconsciously stored in our memory. As he put it (1981, p. 107):

It once happened that a man thought that he had written original verses, and was then found to have read them word for word, long before, in some ancient poet.... I think that dreams often revive former thoughts for us in this way.

As Rosemarie Sand has pointed out (private communication), Leibniz's notion anticipates, to some extent, Freud's dictum that "*The interpretation of dreams is the royal road to a knowledge of the unconscious activities of the mind*" (1900a, p. 608).

Before Freud was born, Hermann von Helmholtz discovered the phenomenon of "unconscious inference" as being present in sensory perception (Ellenberger, 1970, p. 313). For example, we often unconsciously infer the *constancy* of the *physical* size of nearby objects that move away from us when we have *other* distance cues, although their *visual* images decrease in size. Similarly, there can be unconsciously inferred constancy of brightness and colour under changing conditions of illumination, when the light source remains visible. Such unconscious *inferential compensation* for visual discrepancies also occurs, when we transform our *non-Euclidean* (hyperbolic) binocular *visual* space into the "seen" Euclidean physical space (Grünbaum, 1973, pp. 154–157).

Historically, it is more significant that Freud also had other precursors who anticipated some of his key ideas with impressive *specificity*. As he himself acknowledged (1914d, pp. 15–16), Arthur Schopenhauer and Friedrich Nietzsche had speculatively propounded major psychoanalytic doctrines that he himself reportedly developed independently from his clinical observations only thereafter. Indeed, a new German book by the Swiss psychologist Marcel Zentner (1995) traces the foundations of psychoanalysis to the philosophy of Schopenhauer.

Preparatory to my critical assessment of the psychoanalytic enterprise, let me emphasise the existence of major differences between the unconscious processes hypothesised by current cognitive psychology, on the one hand, and the unconscious contents of the mind claimed by psychoanalytic psychology, on the other (Eagle, 1987). These differences will show that the existence of the *cognitive* unconscious clearly fails to support, or even may cast doubt on, the existence of Freud's *psychoanalytic* unconscious. His so-called "*dynamic*" unconscious is the supposed repository of repressed forbidden wishes of a sexual or aggressive nature, whose re-entry or initial entry into consciousness is prevented by the defensive operations of the ego. Though socially unacceptable, these instinctual desires are so imperious and peremptory that they recklessly seek immediate gratification, independently of the constraints of external reality.

Indeed, according to Freud (1900a, pp. 566–567), we would not even have developed the skills needed to engage in cognitive activities, if it had been possible to gratify our instinctual needs without reliance on these cognitive skills. Thus, as Eagle has pointed out:

Freud did not seem to take seriously the possibility that cognition and thought could be inherently programmed to reflect reality and could have their own structure and development—an assumption basic to cognitive psychology. After World War II, the psychoanalyst Heinz Hartmann was driven, by facts of biological maturation discovered *non-psychoanalytically*, to acknowledge in his so-called "ego psychology" that such functions as cognition, memory and

In the cognitive unconscious, there is great rationality in the ubiquitous computational and associative problem-solving processes required by memory, perception, judgment and attention. By contrast, as Freud emphasised, the wish-content of the dynamic unconscious makes it operate in a highly illogical way.

There is a further major difference between the two species of unconscious (Eagle, 1987, pp. 163–165): The dynamic unconscious acquires its content largely from the unwitting repression of ideas from the form they originally had in consciousness. By contrast, in the generation of the processes in the cognitive unconscious, neither the expulsion of ideas and memories from consciousness nor the censorious denial of entry to them plays any role at all. Having populated the dynamic unconscious by means of repressions, Freud reasoned that the use of his new technique of free association could bring these repressions of instinctual wishes, and could thereby bring the repressed ideas back into consciousness *unchanged*. But in the case of the cognitive unconscious, we typically cannot bring into phenomenal consciousness the intellectual processes that are presumed to occur in it, although we can describe them theoretically.

For example, even if my life depended on it, I simply could not bring into my phenomenal conscious experience the elaborate scanning or search-process by which I rapidly come up with the name of the Czarina's lover Rasputin when I am asked for it. Helmholtz's various processes of “unconscious inference” illustrate the same point. By glossing over the stated major difference between the two species of unconscious, some psychoanalysts have claimed their compatibility with the same genus without ado (Shevrin *et al.*, 1992, pp. 340–341). But Eagle (1987, pp. 166–186) has articulated the extensive modifications required in the Freudian notion of the dynamic unconscious, if it is to be made compatible with the cognitive one.

More importantly, some Freudian apologists have overlooked that even after the two different species of the genus “unconscious” are thus made logically *compatible*, the dynamic unconscious as such cannot derive any *credibility* from the presumed existence of the cognitive unconscious. Nonetheless, faced with mounting attacks on their theory and therapy, some psychoanalysts have made just that fallacious claim. Thus, the Chicago analyst Michael Franz Basch (1994, p. 1) reasons in vain that since neurophysiological evidence supports the hypothesis of a *generic* unconscious, “psychoanalytic theory has passed the [epistemological] test with flying colours.” On the contrary, we must bear in mind that evidence for the cognitive unconscious does not, as such, also furnish support for the dynamic unconscious as such.

III. Has psychoanalytic theory become a staple of western culture?

In appraising psychoanalysis, we must also beware of yet another logical blunder that has recently become fashionable: The bizarre argument recently given by a number of American philosophers (e.g., Nagel, 1994a), that the supposed pervasive influence of Freudian ideas in Western culture vouches for the validity of the psychoanalytic enterprise. But this argument is demonstrably untenable (Grünbaum, 1994).

Even its premise that Freudian theory has become part of the intellectual ethos and folklore of Western culture cannot be taken at face value. As the great Swiss scholar Henri Ellenberger (1970, pp. 547–549) has stressed in his monumental historical work *The Discovery of the Unconscious*, the prevalence of vulgarised *pseudo*-Freudian concepts makes it very difficult to determine reliably the extent to which *genuine* psychoanalytic hypotheses have actually become influential in our culture at large. For example, *any* slip of the tongue or other bungled action (*parapraxis*) is typically y

incorrectly called a “Freudian slip.”

But, Freud himself has called attention to the existence of a very large class of lapses or slips whose psychological motivation is simply *transparent* to the person who commits them or to others (1916a, p. 40). And he added commendably that neither he nor his followers deserve any credit for the motivational explanations of such perspicuous slips (1916a, p. 47). In this vein, a psychoanalyst friend of mine provided me with the following example of a *pseudo*-Freudian slip that would, however, be wrongly yet widely called “Freudian”: A man who is at a crowded party in a stiflingly hot room starts to go outdoors to cool off, but is confronted by the exciting view of a woman's *decolleté* bosom and says to her: “Excuse me, I have to get a *breast* of *flesh* air.” Many otherwise educated people would erroneously classify this slip as Freudian for two *wrong* reasons: First, *merely* because it is motivated rather than a purely mechanical *lapsus linguae*, and, furthermore, because its theme is sexual.

Yet what is required for a slip or so-called “parapraxis” to qualify as *Freudian* is that it be motivationally *opaque* rather than transparent, precisely because its psychological motive is *repressed* (1916a, p. 41). As the father of psychoanalysis declared unambiguously (1901b, p. 239): psychoanalysis is to provide an explanation of a parapraxis, “we must not be aware in ourselves of any motive for it. We must rather be tempted to explain it by ‘inattentiveness’, or to put it down to ‘chance’.” And Freud characterised the pertinent explanatory unconscious causes of slips as “motives of displeasure.” Thus, when a young man forgot the Latin word “*aliquis*” in a quotation from Virgil, Freud diagnosed its interfering cause as the man's distressing unconscious fear that his girlfriend had become pregnant by him (1901b, p. 9). *If* that latent fear was actually the motive of the slip, it was surely *not apparent* to anyone.

Once it is clear what is *meant* by a *bona fide* Freudian slip, we need to ask whether there *actually exist* any such slips at all, that is, slips which *appear* to be psychologically *unmotivated* but are actually caused by repressed unpleasant ideas. It is very important to appreciate how difficult it is to provide cogent evidence for such causation. K. Schüttauf *et al.* (1997) claim to have produced just such evidence. They note that, according to psychoanalytic etiologic theory, obsessive-compulsive neurosis is attributable to an unconscious conflict whose repressed component features anal-erotic and sadistic wishes, which are presumably activated by regression. Then they reason that when such conflict-laden material is to be verbalised by obsessive-compulsive neurotics, Freudian theory expects a higher incidence of misspeakings (slips of the tongue) among them than among normal subjects. And these researchers report that all of their findings bore out that expectation.

This investigation by K. Schüttauf *et al.* differs from Bröder's (1995) strategy, which was designed to inquire into “the possible influence of unconscious information-processing on the frequency of specific speech-errors in an experimental setting”. Thus, Bröder and Bredenkamp (1996, Abstract) claim to have produced experimental support for the “weaker Freudian thesis” of verbal slip generation by unconscious, rather than repressed, thoughts: “Priming words that remain unconscious induce misspeaking errors with higher probability than consciously registered ones.”

As for the soundness of the design of Schüttauf *et al.*, Hans Eysenck (private communication with Rosemarie Sand, March 1, 1996; cited by permission to her) has raised several objections: (i) “as the author [Schüttauf] himself acknowledges, this is not an experiment, as ordinarily understood; it is a simple correlational study...correlation cannot be interpreted as causation, which he unfortunately attempts to do,” (ii) The members of the experimental group were severely neurotic, while the control group were normals. But “the proper control group would have been severely [disturbed] neurotics suffering from a different form of neurosis than that of obsessive compulsive behaviour,” (iii) “Freudian theory posits a causal relationship between the anal stage of development and obsessive compulsive neurosis; the author does not even try to document this hypothetical relationship”, (iv) “obsessive-compulsive neurotics suffer from fear of dirt and contamination, so that on those grounds

alone they would be likely to react differentially to stimuli suggesting such contamination.... It is truly commonsensical to say that people whose neurosis consists of feelings of dirt will react differentially to verbal presentations of words related to dirt.”

Naturally, I sympathise with Schüttauf and his co-workers in their avowed effort (Section 4) to escape my criticism (Grünbaum, 1984, pp. 202–205) of an earlier purported experimental confirmation of Freud's theory of slips by M. T. Motley (1980). I had complained that the independent variable Motley manipulated in his speech-error experiments did *not* involve *unconscious* antecedents—but only conscious ones. As Schüttauf *et al.* tell us, precisely in order to escape my criticism of Motley, they relied on Freud's aetiology of obsessive-compulsive neurosis to infer that subjects who exhibit the symptoms of that neurosis fulfil the requirement of harbouring repressions of anal-sadistic wishes. Thus, *only* on that etiologic assumption does their use of compulsive subjects *and* the manipulation of words pertaining to anal-sadistic themata warrant their expectation of a high incidence of verbal slips in this group than among normals.

Surely one could not reasonably expect the authors themselves to have carried out empirical tests of the aetiology on which their entire investigation is *crucially predicated*. But nonetheless Eysenck's demand for such evidence is entirely appropriate: Without independent *supporting* evidence for the aetiology, their test is definitely *not* a test of Freud's theory of slips of the tongue, let alone—as they conclude—a confirmation of it.

Thus, as long as good empirical support for the Freudian scenario is unavailable, we actually do not know whether any *bona fide* Freudian slips exist at all. Just this lack of evidence serves to undermine Nagel's thesis that cultural influence is a criterion of validity. After all, if we have no cogent evidence for the existence of genuinely Freudian slips, then Freud's theory of bungled actions (“*parapraxes*”) might well be false. And if so, it would not contribute one iota to its validity, even if our entire culture unanimously believed in it, and made extensive explanatory use of it: When an ill-supported theory is used to provide explanations, they run the grave risk of being bogus, and its purported insights may well be *pseudo-insights*.

A second example supporting my rejection of Nagel's cultural criterion is furnished by the work of the celebrated art historian Meyer Schapiro of Columbia University. Schapiro saw himself as greatly influenced by Freud in his accounts of the work of such painters as Paul Cézanne, who died in 1906 (Solomon, 1994). Of course, Schapiro never actually put Cézanne on the psychoanalytic couch. But he subjected artists indirectly “to his own [brand of speculative] couch treatment” (Solomon, 1994). In his best known essay, Schapiro “turns the Frenchman into a case history.” Indeed, a recent tribute to Schapiro's transformation of scholarship in art history (Solomon, 1994) says that his “accomplishment was to shake off the dust and open the field to a style of speculation and intellectual bravura that drew...most notably [on] psychoanalysis” (Solomon, 1994, p. 24). Reportedly, “his insights into...the apples of Cézanne” (Solomon, 1994, p. 24) make the point that Cézanne's “depictions of apples contain [in Schapiro's words] ‘a latent erotic sense’.”

But if apples are held to symbolise sex unconsciously for Cézanne or anyone else, why doesn't *anything else* that resembles apples in *some* respect (e.g., being quasi-spherical) do likewise? Yet we learn that Schapiro's 1968 publication “The Apples of Cézanne” is “His best known essay” (p. 25). Alas, if Schapiro's claim that Cézanne was “unwillingly chaste” is to be a psychoanalytic insight gleaned from his art, rather than a documented biographical fact, I must say that Schapiro's psychiatric diagnosis is an instance of what Freud himself deplored as “Wild psycho-analysis” (1910k, pp. 221–227). In any case, *pace* Nagel, such art-historical invocation of Freud, however influential, does nothing, I claim, to enhance the *credibility* of psychoanalysis.

For centuries, even as far back as in New Testament narratives, both physical disease and insanity have been attributed to demonic possession in Christendom, no less than among primitive people.

That demon theory has been used, for example, to explain deafness, blindness, and fever, as well as such psychopathological conditions as epilepsy, somnambulism, and hysteria. Our contemporary medical term “epilepsy” comes from the Greek word “epilepsis” *for seizure*, and reflects etymologically the notion of being seized by a demon. Since exorcism is designed to drive out the devil, it is the supposed *therapy* for demonic possession. In the Roman Catholic exorcist ritual, which has been endorsed by the present Pope and by Cardinal O'Connor of New York, the existence of death is blamed on Satan. And that ritual also survives in baptism as well as in blessing persons and consecrating houses.

How does the strength of the cultural influence of such religious beliefs and practices compare with that of Freud's teachings? Though Freud characterised his type of psychotherapy as “*primus inter pares*” (1933a, p. 157), he conceded sorrowfully: “I do not think our [psychoanalytic] cures can compete with those of Lourdes. There are so many more people who believe in the miracles of the Blessed Virgin than in the existence of the unconscious” (1933a, p. 152). Clearly, the psychoanalytic and theological notions of aetiology and of therapy clash, and their comparative cultural influence cannot cogently decide between them. But, if it *could*, psychoanalysis would be the loser! This alone, claim, is a *reductio ad absurdum* of the thesis that the validity of the psychoanalytic enterprise is assured by its wide cultural influence.

Nor can Nagel buttress that thesis by the dubious, vague declaration that psychoanalysis is a “*extension*” of common sense. The term “*extension*” is hopelessly unable to bear the weight required by his thesis, if *actual* psychoanalytic theory is to square with it. What, for example, is *commonsensical* about the standard psychoanalytic etiologic explanation of male diffidence and social anxiety by repressed adult “*castration anxiety*” (Fenichel, 1945, p. 520) or of a like explanation of a male driver's stopping at a *green* traffic light as if it were red? (Brenner, 1982, pp. 182–183). Common sense rightly treats such explanations incredulously as bizarre, and rightly so: As I have shown (Grünbaum, 1997), these etiologic explanations rest on quicksand, even if we were to grant Freud's oedipal scenario that all adult males unconsciously dread castration by their fathers for having lust after their mothers.

IV. Critique of Freudian and post-Freudian psychoanalysis

Let me now turn to my critique of the core of Freud's original psychoanalytic theory and to a verdict on its fundamental modifications by two major post-Freudian sets of hypotheses called “*self psychology*” and “*object relations theory*.”

The pillars of the avowed “*cornerstone*” of Freud's theoretical edifice comprise several major theses: (1) Distressing mental states induce the operation of a psychic mechanism of repression, which consists in the banishment from consciousness of *unpleasurable* psychic states (1915d, p. 147). (2) Once repression is operative (more or less fully), it not only banishes such negatively charged ideas from consciousness, but plays a *further* crucial multiple causal role: It is *causally necessary* for the pathogenesis of neuroses, the production of our dreams, and the generation of our various sorts of slips (bungled actions). And (3) the “*method of free association*” can identify and lift (undo) the patient's repressions; by doing so, it can identify the pathogenesis of the neuroses, and the generators of our dreams, as well as the causes of our motivationally opaque slips; moreover, by lifting the pathogenic repressions, free association functions therapeutically, rather than only investigatively.

Freud provided two sorts of arguments for his cardinal etiologic doctrine that repressions are the pathogenesis of the neuroses: His earlier one, which goes back to his original collaboration with Josef Breuer, relies on purported *therapeutic successes* from lifting repressions; the later one, which was designed to show that the pathogenic repressions are *sexual*, is drawn from presumed re-enactment

(“transferences”) of infantile episodes in the adult patient's interactions with the analyst during psychoanalytic treatment.

It will be expositoryly expeditious to deal with Freud's earlier etiologic argument within Section below, and to appraise the subsequent one, which goes back to his “Dora” case history of 1905, Section C (Freud, 1905e). But, also for expository reasons, it behooves us to devote a prior Section to his account of the actuation of the hypothesised mechanism of repression by “motives unpleasure.”

A. Negative affect and forgetting

As Freud told us, “The theory of repression is the cornerstone on which the whole structure of psychoanalysis rests. It is the most essential part of it” (1914d, p. 16). The *process* of repression, which consists in the banishment of ideas from consciousness or in denying them entry into it, is itself presumed to be unconscious (1915d, p. 147). In Freud's view, our neurotic symptoms, the manifested contents of our dreams, and the slips we commit are each constructed as “compromises between the demands of a repressed impulse and the resistances of a censoring force in the ego” (1925d, p. 4; 1917, p. 301). By being only such compromises, rather than fulfilments of the instinctual impulse, these products of the unconscious afford only *substitutive* gratifications or outlets. For brevity, one can say, therefore, that Freud has offered a unifying “compromise-model” of neuroses, dreams and parapraxes.

But what, in the first place, is the *motive* or cause that initiates and sustains the operation of this unconscious mechanism of repression *before* it produces its own later effects? Apparently, Freud assumes *axiomatically* that distressing mental states, such as forbidden wishes, trauma, disgust, anxiety, anger, shame, hate, guilt, and sadness—all of which are *unpleasurable*—almost always actuate, and then fuel, *forgetting* to the point of repression. Thus, repression regulates pleasure and unpleasure by defending our consciousness against various sorts of *negative affect*. Indeed, Freud claimed perennially that repression is the paragon among our *defence* mechanisms (Thomä & Kächele, 1987, vol. 1, pp. 107–111). As Freud put it dogmatically: “The tendency to forget what is disagreeable seems to me to be a quite universal one” (1901b, p. 144), and “the recollection of distressing impressions and the occurrence of distressing thoughts are opposed by a resistance” (1901b, p. 146).

Freud tries to disarm an important objection to his thesis that “distressing memories succumb especially easily to motivated forgetting” (1901b, p. 147). He says:

The assumption that a defensive trend of this kind exists cannot be objected to on the ground that one often enough finds it impossible, on the contrary, to get rid of distressing memories that pursue one, and to banish distressing affective impulses like remorse and the pangs of conscience. For we are not asserting that this defensive trend is able to put itself into effect *in every case*.... (p. 147, italics added)

He acknowledges as “also a true fact” that “distressing things are particularly hard to forget” (1901b, pp. 76–77).

For instance, we know from Charles Darwin's Autobiography that his father had developed remarkably retentive memory for painful experiences (cited in Grünbaum, 1994), and that a half-century after Giuseppe Verdi was humiliatingly denied admission to the Milan Music Conservatory, he recalled it indignantly (Walker, 1962, pp. 8–9). Freud himself told us as an adult (1900a, p. 216) that he “can remember very clearly,” from age seven or eight, how his father rebuked him for having relieved himself in the presence of his parents in their bedroom. In a frightful blow to Freud's ego, his father said: “The boy will come to nothing”.

But Freud's attempt here to uphold his thesis of motivated forgetting is *evasive* and *unavailing*.

Since some painful mental states are vividly remembered while others are forgotten or even repressed, I claim that ~~factors different from their painfulness determine whether they are remembered or forgotten~~. For example, personality dispositions or situational variables may in fact be causally relevant. To the great detriment of his theory, Freud never came to grips with the *unfavourable* bearing of this key fact about the mnemonic effects of painfulness on the tenability of the following pillar of his theory of repression: When painful or forbidden experiences are forgotten, the forgetting is tantamount to their repression *due to their negative affect*, and thereby produces neurotic symptoms or other compromise-formations. Thomas Gilovich, a professor of psychology at Cornell University (USA), is now doing valuable work on the conditions under which painful experiences are *remembered*, and on those *other* conditions under which they are forgotten.

The numerous and familiar occurrences of vivid and even obsessive recall of negative experiences pose a fundamental *statistical* and explanatory challenge to Freud that neither he nor his followers have ever met. We must ask (Grünbaum, 1994): Just what is the *ratio* of the forgetting of distressing experiences to their recall, and what *other* factors determine that ratio? Freud gave no statistical evidence for assuming that forgetting them is the *rule*, while remembering them is the exception. Yet as we can see, his theory of repression is devastatingly undermined from the outset if forgettings of negative experiences do not greatly outnumber rememberings statistically. After all, if forgetting is *not* the rule, then what *other* reason does Freud offer for supposing that when distressing experiences are actually forgotten, these forgettings are instances of genuine repression due to affective displeasure? And if he has no such other reason, then, *a fortiori*, he has no basis at all for his pivot etiologic scenario that forbidden or aversive states of mind are usually repressed and thereby cause compromise-formations.

Astonishingly, Freud thinks he can parry this basic statistical and explanatory challenge by an evasive dictum as follows: "...mental life is the arena and battle-ground for mutually opposing purposes [of forgetting and remembering] (1916a, p. 76)...; there is room for both. It is only a question...of what effects are produced by the one and the other" (p. 77). Indeed, just that question cries out for an answer from Freud, if he is to make his case. Instead, he cavalierly left it dangling epistemologically in limbo.

B. The epistemological liabilities of the psychoanalytic method of free association

Another basic difficulty, which besets all three major branches of the theory of repression alike, lies in the epistemological defects of Freud's so-called "fundamental rule" of free association, the supposed microscope and X-ray tomograph of the human mind. This rule enjoins the patient to tell the analyst without reservation whatever comes to mind. Thus it serves as the fundamental method of clinical investigation. We are told that by using this technique to unlock the flood gates of the unconscious, Freud was able to show that neuroses, dreams and slips are caused by repressed motives. Just as Breuer's cathartic use of hypnosis, it is a cardinal thesis of Freud's entire psychoanalytic enterprise that his method of free association has a two-fold major capability, which is both investigative and therapeutic: (i) It can *identify* the unconscious causes of human thoughts and behaviour, both abnormal and normal, and (ii) By overcoming resistances and lifting repressions, it can remove the unconscious pathogens of neuroses, and thus provide therapy for an important class of mental disorders.

But on what grounds did Freud assert that free association has the stunning investigative capability to be *causally probative* for etiologic research in psychopathology? Is it not too good to be true that one can put a psychologically disturbed person on the couch and fathom the aetiology of her or his affliction by free association? As compared to fathoming the causation of major somatic diseases, the

seems almost miraculous, *if at all true*. Freud tells us very clearly (1900a, p. 528) that his argument for his investigative tribute to free association as a means of uncovering the causation of neuroses is at bottom, a *therapeutic* one going back to the cathartic method of treating hysteria. Let me state and articulate his argument.

One of Freud's justifications for the use of free association as a *causally probative* method of dream investigation leading to the identification of the repressed dream thoughts, he tells us (1900a, p. 528) is that it "is identical with the procedure [of free association] by which we resolve hysterical symptoms; and there the correctness of our method [of free association] is warranted by the coincident emergence and disappearance of the symptoms...." But, as I have pointed out elsewhere (Grünbaum, 1993a, pp. 25–26) his original German text here contains a confusing slip of the pen. As we know, the patient's symptoms hardly first emerge simultaneously with their therapeutic dissipation. Yet Strachey translated Freud correctly as having spoken of "the coincident emergence and disappearance of the symptoms". It would seem that Freud means to speak of the *resolution* (German: *Auflösung*), rather than of the emergence (*Auftauchen*), of the symptoms as coinciding with their therapeutic dissipation. Now, for Freud, the "resolution of a symptom", in turn, consists of using free association to uncover the repressed pathogen that enters into the compromise-formation which is held to constitute the symptom. This much, then, is the statement of Freud's appeal to therapeutic success to vouch for the "correctness of our method" of free association as causally probative for etiologic research in psychopathology.

To articulate the argument adequately, however, we must still clarify Freud's original basis for claiming that (unsuccessful) repression is indeed the pathogen of neurosis. Only then will he have made his case for claiming that free association is etiologically probative, because it is uniquely capable of uncovering repressions. The pertinent argument is offered in Breuer and Freud's "Preliminary Communication" (1895d, pp. 6–7). There they wrote (p. 6, italics in original):

For we found, to our great surprise at first, that *each individual hysterical symptom immediately and permanently disappeared when we had succeeded in bringing clearly to light the memory of the event by which it was provoked and in arousing its accompanying affect, and when the patient had described that event in the greatest possible detail and had put the affect into words*. Recollection without affect almost invariably produces no result. The psychological process which originally took place must be repeated as vividly as possible; it must be brought back to its *status nascendi* and then given verbal utterance.

Breuer and Freud make an important comment on their construal of this therapeutic finding:

It is plausible to suppose that it is a question here of unconscious suggestion: the patient expects to be relieved of his sufferings by this procedure, and it is this expectation, and not the verbal utterance, which is the operative factor. This, however, is not so. (p. 7)

And their avowed reason is that, in 1881, i.e. in the "pre-suggestion" era, the cathartic method was used to remove *separately* distinct symptoms, "which sprang from separate causes" such that any one symptom disappeared only after the cathartic ("abreactive") lifting of a *particular* repression. But Breuer and Freud do not tell us why the likelihood of placebo effect should be deemed to be low when several symptoms are wiped out *seriatim* than in the case of getting rid of only one symptom. Thus, as I have pointed out elsewhere (Grünbaum, 1993a, p. 238) to discredit the hypothesis of a placebo effect, it would have been essential to have comparisons with treatment outcome from a suitable control group whose repressions are *not* lifted. If that control group were to fare equally well, treatment gains from psychoanalysis would then be placebo effects after all.

In sum, Breuer and Freud inferred that the therapeutic removal of neurotic symptoms was produced by the cathartic lifting of the patient's previously ongoing repression of the pertinent traumatic memory, not by the therapist's suggestion or some other placebo factor (See Grünbaum, 1993a, ch.

for a very detailed analysis of the placebo-concept). We can codify this claim as follows:

T. Therapeutic Hypothesis: Lifting repressions of traumatic memories cathartically is *causally relevant* to the disappearance of neuroses.

As we saw, Breuer and Freud (p. 6) reported the immediate and permanent disappearance of each hysterical symptom after they cathartically lifted the repression of the memory of the trauma that occasioned the given symptom. They adduce this “evidence” to draw an epoch-making inductive *etiological* inference (p. 6), which postulates “a causal relation between the determining [repression of the memory of the] psychical trauma and the hysterical phenomenon”. Citing the old scholastic dictum “*Cessante causa cessat effectus*” (“when the cause ceases, its effect ceases”), they invoke its contrapositive (p. 7), which states that as long as the effect (symptom) persists, so does its cause, (the repressed memory of the psychical trauma). And they declare just that to be the pattern of the pathogenic action of the repressed psychical trauma. This trauma, we learn, is *not* a mere *precipitating* cause. Such a mere “*agent provocateur*” just releases the symptom, “which thereafter leads a independent existence”. Instead, “the [repressed] memory of the trauma...acts like a foreign body which long after its entry must continue to be regarded as an agent that is still at work” (p. 6).

The upshot of their account is that their observations of positive therapeutic outcome upon the abreactive lifting of repressions, which they interpret in the sense of their therapeutic hypothesis, spelled a paramount etiologic moral as follows:

E. Etiologic Hypothesis: An ongoing repression accompanied by affective suppression is causally necessary for the initial pathogenesis *and* persistence of a neurosis.

(This formulation of the foundational aetiology of psychoanalysis supersedes the one I gave at the hands of a suggestion by Carl Hempel and Morris Eagle [in Grünbaum, 1984, p. 181, last paragraph]. The revised formulation here is faithful to Breuer and Freud's reference to “accompanying affect” [p. 6] à propos of the traumatic events whose repression occasioned the symptoms.)

Clearly, this etiologic hypothesis *E* permits the *valid deduction* of the therapeutic finding reported by Breuer and Freud as codified in their therapeutic hypothesis *T*: the cathartic lifting of the repressions of traumatic memories of events that occasion symptoms engendered the disappearance of the symptoms. And, as they told us explicitly (p. 6), this therapeutic finding is their “evidence” for their cardinal etiologic hypothesis *E*.

But I maintain that this inductive argument is vitiated by what I like to call the “*fallacy of crucial hypothetico-deductive ('H-D') pseudo-confirmation*”. Thus note that the remedial action of aspirin consumption for tension-headaches does not lend H-D support to the outlandish etiologic hypothesis that a hemolytic aspirin-deficiency is a causal *sine qua non* for having tension headaches, although such remedial action is validly deducible from that bizarre hypothesis. Twenty-five years ago, Wesley Salmon called attention to the fallacy of inductive causal inference from mere valid H-D deducibility by giving an example in which a deductively valid pseudo-explanation of a man's avoiding pregnancy can readily give rise to an H-D pseudo-confirmation of the addle-brained attribution of his non-pregnancy to his consumption of birth-control pills. Salmon (1971, p. 34) states the fatuous pseudo-explanation:

John Jones avoided becoming pregnant during the past year, for he had taken his wife's birth control pills regularly, and every man who regularly takes birth control pills avoids pregnancy.

Plainly, this deducibility of John Jones's recent failure to become pregnant from the stated premises does not lend any credence at all to the zany hypothesis that this absence of pregnancy is *causally attributable* to his consumption of birth control pills. Yet it is even true that any men who consum

such pills *in fact* never do become pregnant. Patently, as Salmon notes, the fly in the ointment is that men just do not become pregnant, whether they take birth control pills or not.

His example shows that neither the empirical truth of the deductively inferred conclusion and of the pertinent initial condition concerning Jones nor the deductive validity of the inference can provide bona fide confirmation of the causal hypothesis that male consumption of birth control pills prevents male pregnancy: That hypothesis would first have to meet other epistemic requirements, which it manifestly cannot do.

Crude H-D confirmationism is a paradise of spurious causal inferences, as illustrated by Breuer and Freud's unsound etiologic inference. Thus, psychoanalytic narratives are replete with the belief that a hypothesised etiologic scenario embedded in a psychoanalytic narrative of an analysand's affliction is *made credible* merely because the postulated aetiology then permits the logical deduction of a probabilistic inference of the neurotic symptoms to be explained.

Yet some apologists offer a facile excuse for the fallacious H-D confirmation of a causal hypothesis. We are told that the hypothesis is warranted by an "inference to the best explanation" (Harman, 1965). But in a careful new study, Wesley Salmon (2001) has argued that "the characterization of nondemonstrative inference as inference to the best explanation serves to muddy the waters...by fostering confusion" between two sorts of why-questions that Hempel has distinguished: *Explanation*-seeking questions as to why something is the case, and *confirmation*-seeking why-questions as to why a hypothesis is *credible*. Thus, a hypothesis that is pseudo-confirmed by some data cannot be warranted qua being "the only [explanatory] game in town". Alas, "best explanation"-sanction was claimed for psychoanalytic etiologies to explain and treat the destructive behaviour of sociopaths *to no avail* for years (cf. Cleckley, 1988, Section Four, esp. pp. 238–239 and 438–439).

I can now demonstrate the multiple failure of Freud's therapeutic argument for the etiologic probativeness of free association in psychopathology, no matter how revealing the associativelike contents may otherwise be in regard to the patient's psychological preoccupations and personal dispositions. Let us take our bearings and first encapsulate the structure of his therapeutic argument.

First, Freud inferred that the therapeutic disappearance of the neurotic symptoms is *causally attributable* to the cathartic lifting of repressions *by means of the method free associations*. Relying on this key therapeutic hypothesis, he then drew two further major theoretical inferences: (i) The seeming removal of the neurosis by means of cathartically *lifting* repressions is good inductive evidence for postulating that repressions accompanied by affective suppression are themselves *causally necessary* for the very existence of a neurosis (1895d, pp. 6–7), and (ii) Granted that such repressions are the essential causes of neurosis, *and* that the method of free association is uniquely capable of uncovering these repressions, this method is uniquely competent *to identify the causes* or pathogens of the neuroses. (Having convinced himself of the causal probativeness of the method of free associations on therapeutic grounds in the case of those neuroses he believed to be successfully treatable, Freud also felt justified in deeming the method reliable as a means of unearthing the etiologies of those *other* neuroses—the so-called "narcissistic ones", such as paranoia—which he considered psychoanalytically *untreatable*.)

But the argument fails for the following several reasons: In the first place, the durable therapeutic success on which it was predicated did not materialise (Borch-Jacobsen, 1996), as Freud was driven to admit both early and very late in his career (1925d, p. 27; 1937c, pp. 23, 216–253). But even in so far as there was transitory therapeutic gain, we saw that Freud *failed* to rule out a rival hypothesis that undermines his attribution of such gain to the lifting of repressions by free association: The ominous hypothesis of placebo effect, which asserts that treatment ingredients *other than* insight into the patient's repressions—such as the mobilisation of the patient's hope by the therapist—are responsible

for any resulting improvement (Grünbaum, 1993a, ch. 3). Nor have other analysts ruled out the placebo hypothesis during the past century. A case in point is a 45-page study “On the Efficacy of Psychoanalysis” (Bachrach *et al.*, 1991), published in the official *Journal of the American Psychoanalytic Association*. Another is the account of analytic treatment process by (Vaughan & Roose, 1995).

Last, but not least, the repression-aetiology is evidentially ill-founded, as we saw earlier and will see further in the next Section C. It is unavailing to the purported *etiologic* probativeness of free associations that they may lift repressions, since Freud failed to show that the latter are pathogenic. In sum, Freud's argument has forfeited its premises.

C. Freud's etiologic transference argument

Now let us consider Freud's argument for his cardinal thesis that *sexual* repressions in particular are the pathogens of all neuroses, an argument he deemed “decisive”. Drawing on my earlier writings (1990, pp. 565–567; 1993a, pp. 152–158), we shall now find that this argument is without merit.

According to Freud's theory of transference, the patient *transfers* onto his psychoanalyst feelings and thoughts that originally pertained to important figures in his or her earlier life. In this important sense, the fantasies woven around the psychoanalyst by the analysand, and quite generally the latter's conduct toward his or her doctor, are hypothesised to be *thematically recapitulatory* of childhood episodes. And by thus being recapitulatory, the patient's behaviour during treatment can be said to exhibit a thematic kinship to such very early episodes. Therefore, when the analyst interprets the supposed re-enactments, the ensuing interpretations are called “transference interpretations.”

Freud and his followers have traditionally drawn the following highly questionable causal inference: Precisely in virtue of being thematically recapitulated in the patient-doctor interaction, the hypothesised earlier scenario in the patient's life can cogently be held to have originally been a *pathogenic* factor in the patient's affliction. For example, in his case history of the Rat Man, Freud (1909d) infers that a certain emotional conflict had originally been the precipitating cause of the patient's inability to work, merely because this conflict had been thematically re-enacted in a fantasy the Rat Man had woven around Freud during treatment.

Thus, in the context of Freud's transference interpretations, the thematic re-enactment is claimed to show that the early scenario had originally been *pathogenic*. According to this etiologic conclusion, the patient's thematic re-enactment in the treatment setting is also asserted to be *pathogenically* recapitulatory by being pathogenic in the adult patient's here-and-now, rather than only thematically recapitulatory. Freud (1914d, p. 12) extols this dubious etiologic transference argument in his “History of the Psycho-Analytic Movement,” claiming that it furnishes the most unshakable proof for his sexual aetiology of all the neuroses:

The fact of the emergence of the transference in its crudely sexual form, whether affectionate or hostile, in every treatment of a neurosis, although this is neither desired nor induced by either doctor or patient, has always seemed to me the most irrefragable proof [original German: “*unerschütterlichste Beweis*”] that the source of the driving forces of neurosis lies in sexual life [sexual repressions]. This argument has never received anything approaching the degree of attention that it merits, for if it had, investigations in this field would leave no other conclusion open. As far as I am concerned, this argument has remained the decisive one, over and above the more specific findings of analytic work.

On the contrary, the patient's thematically recapitulatory behaviour toward his doctor *does not show* that it is also *pathogenically* recapitulatory. How, for example, does the re-enactment, during treatment, of a patient's early conflict show at all that the original conflict had been pathogenic in the first place? Quite generally, how do transference phenomena focusing on the analyst show that a presumed current replica of a past event is *pathogenic* in the here-and-now?

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